

**Could the Bubble in U.S. House Prices
Have Been Detected in Real Time?**

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Could the Bubble in U.S. House Prices Have Been Detected in Real Time?*

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Abstract

I explore whether time-series methods exploiting the long-run equilibrium properties of the housing market might have detected the disequilibrium in U.S. house prices which pre-dated the Great Recession as it was building up. Based on *real-time* data, I show that a VAR in levels identified as in Uhlig (2003, 2004) would have detected the disequilibrium with high confidence by the Summer of 2004, with the estimated extent of overvaluation peaking at about 15 per cent immediately before the crisis. These results demonstrate that disequilibria in the prices of at least one asset class—housing—*can* indeed be robustly detected as they are building up.

Conceptually in line with Cochrane’s (1994) analysis for consumption and GNP, and dividends and stock prices, a key factor in order to robustly identify the transitory component of real house prices is applying Uhlig-style identification to *real rents*, which are cointegrated with house prices, and are comparatively much closer to the common stochastic trend. Directly focusing on house prices themselves, on the other hand, produces less robust results.

Keywords: Structural VARs; unit roots; cointegration; long-run restrictions; medium-run identification; Great Recession; housing bubbles.

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‘[...] it is not, as Mr Greenspan argues, impossible to identify bubbles. When prices have lost touch with fundamentals and there are other signs of excess, such as rapid credit growth, alarm bells should ring. [...] Moreover, central banks do not have to be certain they have identified a bubble before they act. Monetary policy has constantly to deal with uncertainty—such as the size of the output gap. Uncertainty is a reason for responding cautiously, but not for doing nothing.’

—From ‘Monetary Myopia’, in *The Economist*, January 12, 2006

1 Introduction

In spite of the crucial role played by the unravelling of a disequilibrium in U.S. house prices in triggering the recent financial crisis and the subsequent Great Recession, quite surprisingly no work has been devoted, so far, to developing a rigorous econometric framework which might allow to detect such disequilibria in real time. This is in sharp contrast to the vast effort which has instead been devoted, e.g., to expanding DSGE models in order to incorporate a financial and a banking sector. As a consequence, although policymakers often openly worry that housing markets may be dangerously out of equilibrium, and may be heading into ‘bubble territory’,¹ they simply have no way of assessing such a possibility within a statistical framework, and are therefore compelled to resort to intuitively sensible, but ultimately quite rough indicators such as the rent-price ratio, or the ratio between house prices and incomes.

In this paper I explore whether time-series methods exploiting the *long-run equilibrium* properties of the housing market might have detected the disequilibrium in U.S. house prices which pre-dated the Great Recession as it was building up. Based on *real-time* data, I show that a VAR in levels identified as in Uhlig (2003, 2004) would have detected the disequilibrium with very high confidence by the Summer of 2004, with the estimated extent of overvaluation peaking at about 15 per cent during the months immediately before the outbreak of the crisis. Cointegrated VARs identified *via* long-run restrictions, on the other hand, produce fragile and uniformly weaker results.²

My results demonstrate, by example, that disequilibria in the prices of at least one asset class—housing—*can* indeed be robustly detected as they are building up. Conceptually in line with Cochrane’s (1994) analysis for consumption and GNP, and dividends and stock prices, a key factor in order to reliably and robustly identify the transitory component of real house prices is focusing on *real rents*, which are

¹In this paper I use the term ‘house price bubble’ *uniquely* to mean ‘a positive, large and transitory deviation of real house prices from their long-run equilibrium value’. The notion of ‘bubble’ used herein is therefore a strictly *statistical* one, and it does not have any connotation in terms of (e.g.) the deviation being a ‘rational bubble’, as opposed to one due to non strictly rational factors.

²This is conceptually in line with Uhlig’s (2003, 2004) criticism of long-run restrictions, which motivated his proposed alternative identification strategy.

cointegrated with house prices, and are comparatively much closer to the common stochastic trend. Specifically, when Uhlig (2003, 2004)-style identification is used in order to extract the single most powerful shock at long horizons for real rents, *all* horizons greater than or equal to 10 years ahead produce near numerically identical results for the transitory component of real house prices. Intuitively, this reflects the fact that real rents are, to a close approximation (and up to a scale factor), the stochastic trend of real house prices, so that all horizons which are ‘not too short’ allow to effectively capture the unit root in house prices. By contrast, when this approach is used in order to extract the most powerful shock for house prices themselves, alternative long horizons used for identification sometimes produce materially different results.

The fact that the methodology analyzed herein would have allowed to detect the disequilibrium in U.S. house prices by the Summer of 2004 does *not* automatically imply that this would have called for countervailing measures on the part of the Federal Reserve: Since in the two previous identified episodes—in the second halves of the 1970s and of the 1980s, respectively—over-valuations of 10 to 15 per cent had not led to dramatic recessions, this evidence hardly suggests that, as of 2004, U.S. policymakers might have been induced to act in order to reign in the disequilibrium. Another way of saying this is that the lack of any countervailing measure on the part of the FED during the period leading up to the crisis (in terms of either regulatory interventions, or running a marginally tighter monetary policy) does not automatically follow from the U.S. central bank not having explored, back then, the potential usefulness of the methodology analyzed herein:³ As discussed, e.g., by then-Chairman Bernanke, in order to find an instance in which the unravelling of a disequilibrium in house prices had indeed had a negative, material impact on the economy one had to reach back to the Great Depression.

The paper is organized as follows. The next two sections discuss the related literature, and analyze the unit root and cointegration properties of the data. Section 4 explores the size of the unit root component of real house prices. Section 5 presents evidence from either cointegrated structural VARs identified *via* long-run restrictions, or VARs in levels identified as in Uhlig (2003, 2004). In Section 6 I perform a *partly* real-time exercise—in which I use real-time data for all series except real rents and the supply of homes (for which such data are not available before 2011)—in order to explore whether the methodology analyzed herein would have detected the disequilibrium in U.S. house prices in real time. Section 7 studies the data revision process for real rents and the supply of homes based on the real-time data available since 2011. Based on these results, in Section 8 I then perform a real-time exercise in which I consider several ‘worst case scenarios’ for the extent of revision noise for real rents and the supply of homes. To anticipate, unless, during the period in which the disequi-

³An important point to stress is that, by that time, *both* Uhlig (2003, 2004)-style identification, *and* the advantages of working with VARs in levels—first extensively discussed by Sims, Stock, and Watson (1990)—were already known to the profession.

librium in house prices was building up, the revision noise had been (i) significantly greater than it has historically been since 2011, and (ii) consistently one-sided (i.e., systematically positive) for both real rents and the supply of homes, results would have been near-numerically identical to those of Section 6 (that is: the VAR-based methodology analyzed herein would indeed have detected the disequilibrium in U.S. house prices by the Summer of 2004). Intuitively, this has to do with the fact that the revision noise for real rents—which, as previously mentioned, are the key variable for the purpose of identifying the stochastic trend in house prices—is very small, so that the initial release is very close to the final estimate. Section 9 concludes.

2 Related Literature

Although the present work explores the dynamics of real house prices, the single paper it is conceptually closest to is, in fact, Cochrane’s (1994) exploration of how to disentangle permanent and transitory components of GNP and stock prices by exploiting the informational content of consumption and dividends, respectively. Conceptually in line with Cochrane (1994), Gallin (2008) explored the relationship between U.S. real house prices and real rents within a bivariate cointegrated VECM framework, but he limited himself to documenting cointegration between the two series, and the forecasting power of the rent/price ratio for future movements in real house prices, whereas he did not explore any of the issues I address in the present work.

With very few exceptions (discussed below) the problem of assessing the extent of over- or under-valuation of real house prices has been largely ignored by the academic literature. On the other hand, the issue is regularly discussed in the financial press (see in particular the analysis provided by *The Economist* at the quarterly frequency), and it is intensely analyzed within policymaking institutions,⁴ based on two simple indicators: the rent/price ratio and the ratio between house prices and incomes. Although both measures are intuitively sensible, there are two fundamental problems with this approach. *First*, it is very much *ad hoc*, as it is not based on any rigorous statistical framework exploiting the long-run equilibrium properties of the housing market. *Second*, the fact that such an approach is not based on econometric methods implies that it cannot produce measures of uncertainty around its ‘estimate’.⁵ As a result, it does not allow to make probabilistic statements such as ‘There is an x per cent probability that real house prices are over-valued by at least y per cent.’. Being

⁴As a typical example of work done at the *Bank for International Settlements*, see Scatigna, Szemere, and Tsatsaronis (2014). Analogous evidence is produced, e.g., by the *International Monetary Fund*’s ‘Global Housing Watch’ (see at <http://www.imf.org>).

⁵A third problem with the traditional approach is that it is not infrequent for the two statistics to provide starkly different, and sometimes even conflicting signals. The August 30, 2014 issue of *The Economist*, for example, reported that real house prices in New Zealand were over-valued by 30 per cent based on incomes, and by 74 per cent based on rents. In China, they were over-valued by 7 per cent based on rents, but they were *under*-valued by 38 per cent based on incomes.

able to provide such an assessment would be of obvious relevance within a policy context, as (e.g.) it might play an important role in the decision to change, or not to change, specific macro-prudential instruments such as caps to the loan-to-value ratio. Developing an econometric framework which allows to make such probabilistic statements is a key objective of the present work.

Iacoviello (2000) explored housing market dynamics in six European countries based on cointegrated structural VARs identified *via* long-run restrictions. A key difference with the present work is that he did not compute equilibrium values for real house prices, as his analysis was mostly focused on IRFs and variance decompositions. A potentially important limitation of Iacoviello’s work is that, as I discuss in Section 5 below, cointegrated VARs identified *via* long-run restrictions tend to produce fragile results.

Himmelberg, Mayer, and Sinai (2005) constructed measures of the imputed annual rental cost of owning a home for several U.S. local housing markets, and used it in order to assess the extent of over- or under-valuation of real house prices. Their overall conclusion was that

‘[...] in 2004, prices looked reasonable. Only a few cities, such as Miami, Fort Lauderdale, Portland (Oregon) and, to a degree, San Diego, had valuation ratios approaching those of the 1980s’.

As we will see, this conclusion is radically at odds with the results from my real-time exercise, which identifies, with very high confidence, an over-valuation of U.S. real house prices by the Summer of 2004.

3 Unit Root and Cointegration Properties of the Data

Figure 1 plots the eight series I will be working with: real house prices and real rents; housing starts, the monthly ‘supply of homes’,⁶ and overall employees in construction; and inflation, the Federal Funds rate, and the 30-year mortgage rate. As for the Federal Funds rate, when I work with the longer sample up to August 2017, for the ZLB period I consider Wu and Xia’s (2016) ‘shadow Federal Funds rate’ (which in the second panel of Figure 1 is represented by the blue line), in order to better capture the monetary policy stance. (From now on, ‘*ex post* real rate’ should be regarded as

⁶The ‘monthly supply of homes in the United States’ is described by the U.S. Bureau of the Census as ‘[...] the ratio of houses for sale to houses sold. This statistic provides an indication of the size of the for sale inventory in relation to the number of houses currently being sold. The months’ supply indicates how long the current for sale inventory would last given the current sales rate if no additional new houses were built.’ This variable is therefore essentially a measure of the excess supply in the housing market.

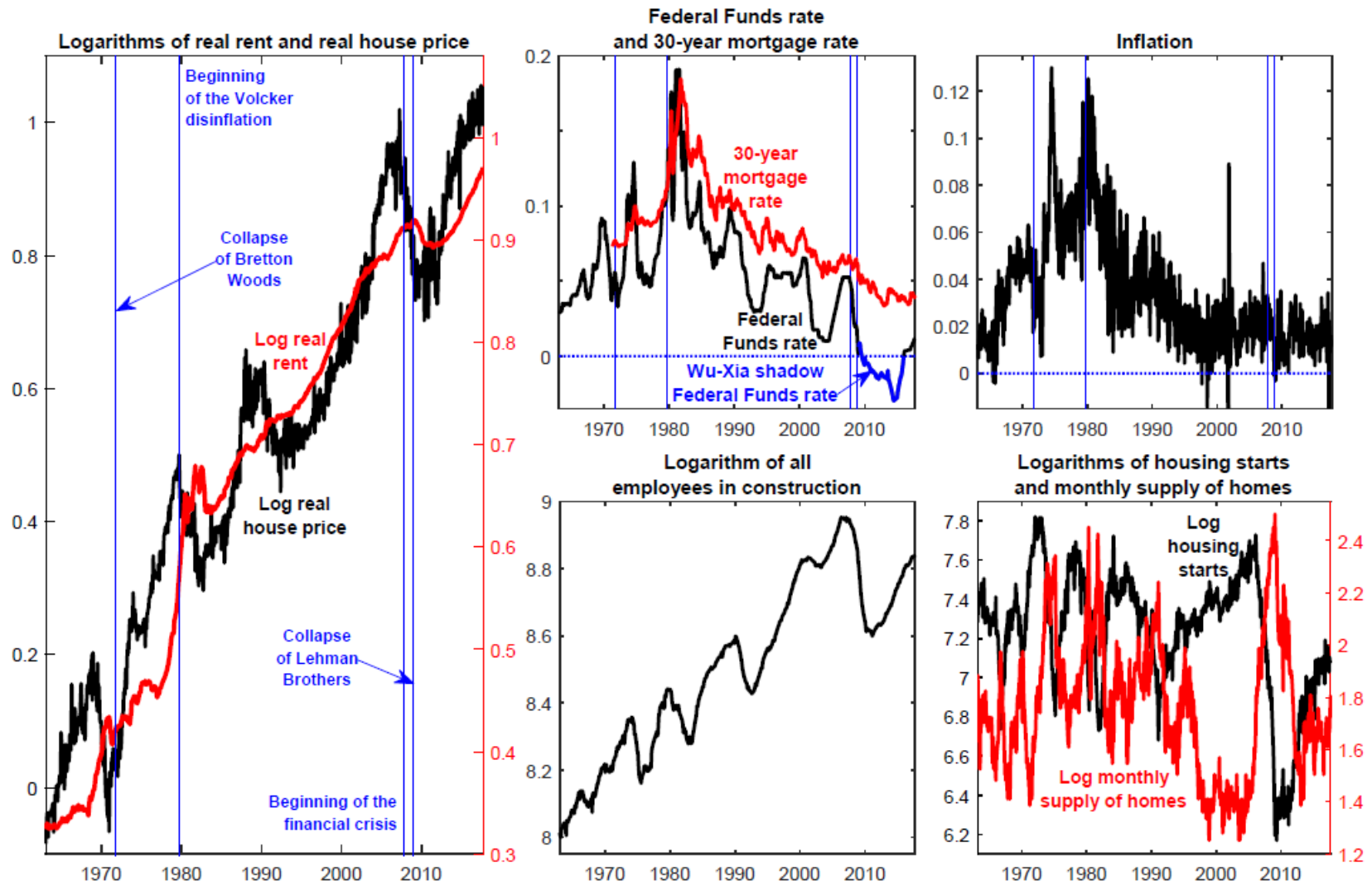


Figure 1 The raw data (January 1963-August 2017)

shorthand for ‘*ex post* real Federal Funds rate’. By the same token, ‘PCE deflator’ will be shorthand for ‘core PCE deflator’.) For details on the data, see Appendix 1.

I have chosen to work with overall *employees*—rather than hours worked⁷—in the construction sector for two reasons. *First*, the two alternative sets of results based on employees, and on hours worked respectively, for either the full sample period April 1971-August 2017, or the shorter sample excluding the ZLB period (April 1971-November 2008), are near-identical.⁸ *Second*, and crucially, real-time data for average hours worked in construction—which are needed in order to compute overall hours worked in the construction sector (see previous footnote)—are not available.

3.1 Unit root tests

Table 1 reports bootstrapped p -values for Elliot, Rothenberg, and Stock (1996) unit root tests.⁹ The table reports results for either the two sample periods I will consider in Section 5 (April 1971-November 2008, and April 1971-August 2017), and, only for house prices and rents (which are both available at least since January 1963), and the real *ex post* Federal Funds rate, for the period January 1963-August 2017. For either real house prices, real rents, or employees in construction, which exhibit obvious trends, the tests are based on models including an intercept and a time trend.¹⁰ For the other series, tests are based on models including an intercept, but no time trend.

Evidence of a unit root is uniformly strong for real rents and real house prices, the Federal Funds rate, the 30-year mortgage rate, inflation, employees in construction, and housing starts. Evidence for the real *ex post* Federal Funds rate is mixed based on either of the samples starting in 1971, whereas a unit root is strongly rejected based on the longer sample January 1963-August 2017. Further, for the sample January 1959-August 2017¹¹ the rejection is even stronger, with the p -values ranging between 0.000 and 0.065. This clearly suggests that the real *ex post* Federal Funds rate should be regarded as $I(0)$, and that the mixed and inconclusive results obtained based on shorter samples should likely be regarded as the figment of using comparatively small sample periods. This is consistent with the fact that, as I discuss in the next subsection, Johansen’s tests produce strong evidence of cointegration between real rents

⁷As discussed in Appendix A, the series for overall hours worked in the construction sector has been constructed as in Iacoviello and Neri (2010), that is, as the product of all employees in the construction sector and average hours worked in construction.

⁸Results based on hours worked are not reported here for reasons of space, but they are available upon request.

⁹For either series, p -values have been computed by bootstrapping 10,000 times estimated ARIMA($p,1,0$) processes. In all cases, the bootstrapped processes are of length equal to the series under investigation. As for the lag order, p , since, as it is well known, results from unit root tests may be sensitive to the specific lag order which is being used, for reasons of robustness I consider four alternative lag orders, 3, 6, 9 and 12 months.

¹⁰The reason for including a time trend is that, as discussed e.g. by Hamilton (1994, pp. 501), the model used for unit root tests should be a meaningful one also under the alternative.

¹¹The PCE deflator is only available since January 1959.

Table 1 United States: bootstrapped p-values for Elliot, Rothenberg, and Stock unit root tests^a				
	Lag order:			
	$p=3$	$p=6$	$p=9$	$p=12$
	<i>January 1963-August 2017</i>			
Log real rent	0.942	0.874	0.647	0.514
Log real house price	0.297	0.416	0.183	0.073
<i>Ex post</i> real Federal Funds rate	0.000	0.037	0.064	0.086
	<i>April 1971-November 2008</i>			
Log real rent	0.833	0.755	0.488	0.287
Log real house price	0.351	0.257	0.119	0.107
Federal Funds rate	0.312	0.485	0.258	0.454
30-year conventional mortgage rate	0.767	0.762	0.679	0.629
Inflation	0.031	0.479	0.539	0.579
<i>Ex post</i> real Federal Funds rate	0.000	0.067	0.088	0.102
Log housing starts	0.447	0.223	0.091	0.144
Log monthly supply of homes	0.310	0.521	0.425	0.351
Log all employees in construction	0.564	0.121	0.074	0.039
	<i>April 1971-August 2017</i>			
Log real rent	0.787	0.673	0.434	0.305
Log real house price	0.235	0.220	0.080	0.063
Federal Funds rate	0.384	0.551	0.333	0.522
30-year conventional mortgage rate	0.856	0.871	0.819	0.778
Inflation	0.011	0.359	0.446	0.500
<i>Ex post</i> real Federal Funds rate	0.003	0.113	0.171	0.181
Log housing starts	0.288	0.142	0.073	0.113
Log monthly supply of homes	0.079	0.141	0.086	0.056
Log all employees in construction	0.695	0.273	0.222	0.154
^a Based on 10,000 bootstrap replications of estimated ARIMA processes.				

and real house prices: If the *ex post* real rate were $I(1)$, theory suggests that this *should not* be the case.¹² The reason is that, in equilibrium, the ratio between rents and house prices should be equal to the real interest rate. Since, as discussed, both series are $I(1)$, this implies that if the real rate is $I(0)$, rents and house prices ought to be cointegrated, whereas if it is $I(1)$ they ought not to. The fact that Johansen's tests detect strong evidence of cointegration between real rents and real house prices is therefore in line with the rejection of a unit root in the real *ex post* Federal Funds rate based on the longest period. Finally, evidence of a unit root in the supply of homes is strong based on the sample period excluding the ZLB, whereas it is weak based on the full sample period April 1971-August 2017. In what follows I will therefore exclude this series from the cointegrated SVAR for the latter period, whereas I will include it in the SVAR for the former one. In the end, however, this will turn out to be irrelevant, because, as mentioned in the Introduction, cointegrated SVARs tend to produce weak and fragile results. On the other hand, I will include the supply of homes in the VARs in levels, since this approach does not require to take a stand on the series' order of integration (see, e.g., the discussion in Hamilton (1994)).

3.2 Cointegration tests

Table 2 reports results from Johansen's cointegration tests. The main results can be summarized as follows.

Evidence of cointegration between real rents and real house prices is strong based on the sample period January 1963-August 2017, and slightly less so based on the period April 1971-August 2017, although it is still detected at the 10 per cent level. Interestingly, the null of no cointegration is *not* rejected based on the period April 1971-November 2008, which saw the building up of the disequilibrium in house prices which pre-dated the Great Recession, but only saw the *very beginning* of its unravelling. These results naturally suggest that house prices and rents are indeed cointegrated, but their temporary divergence during the period in which the 'bubble' was inflating prevents Johansen's tests from detecting cointegration based on the shortest sample period. On the other hand, increasing the sample size, either backward, or especially forward in time, provides additional information which allows the tests to detect cointegration.

Results from the eight-variables system for the period April 1971-November 2008 point towards six cointegration vectors based on the trace test, and four based on the maximum eigenvalue test. In what follows, I will work under the assumption that the system features four cointegration vectors. By the same token, results from the seven-variables system for the period April 1971-August 2017 point towards six cointegration vectors based on the trace test, and four based on the maximum eigenvalue test. Likewise, in what follows I will work under the assumption that the system features four cointegration vectors.

¹²I wish to thank Matteo Iacoviello for an extremely useful email exchange on this issue.

Table 2 United States: results from Johansen's cointegration tests^a

	Trace tests of the null of no cointegration against the alternative of h or more cointegrating vectors:						
	$h = 1$	$h = 2$	$h = 3$	$h = 4$	$h = 5$	$h = 6$	$h = 7$
	<i>Log real house price and log real rent:</i>						
<i>January 1963-August 2017</i>	17.69 (0.041)	—	—	—	—	—	—
<i>April 1971-November 2008</i>	13.10 (0.148)	—	—	—	—	—	—
<i>April 1971-August 2017</i>	17.51 (0.062)	—	—	—	—	—	—
	<i>Eight-variables system:</i>						
<i>April 1971-November 2008</i>	392.64 (0.000)	234.97 (0.000)	141.69 (0.000)	75.56 (0.000)	38.174 (0.027)	19.93 (0.050)	7.55 (0.147)
	<i>Seven-variables system excluding supply of houses:</i>						
<i>April 1971-August 2017</i>	295.29 (0.000)	182.02 (0.000)	97.16 (0.000)	49.13 (0.000)	21.96 (0.025)	9.70 (0.032)	—
	<i>Maximum eigenvalue tests of h versus $h+1$ cointegrating vectors:</i>						
	<i>0 versus 1</i>	<i>1 versus 2</i>	<i>2 versus 3</i>	<i>3 versus 4</i>	<i>4 versus 5</i>	<i>5 versus 6</i>	<i>6 versus 7</i>
	<i>Log real house price and log real rent:</i>						
<i>January 1963-August 2017</i>	16.80 (0.036)	—	—	—	—	—	—
<i>April 1971-November 2008</i>	9.35 (0.321)	—	—	—	—	—	—
<i>April 1971-August 2017</i>	15.05 (0.089)	—	—	—	—	—	—
	<i>Eight-variables system:</i>						
<i>April 1971-November 2008</i>	157.68 (0.000)	93.28 (0.000)	66.13 (0.000)	37.39 (0.040)	18.24 (0.567)	—	—
	<i>Seven-variables system excluding supply of houses:</i>						
<i>April 1971-August 2017</i>	113.27 (0.000)	84.86 (0.000)	48.03 (0.003)	27.17 (0.096)	12.25 (0.589)	—	—

^a Bootstrapped p -values (in parentheses) are based on 10,000 bootstrap replications, based on Cavaliere *et al.*'s (2012) methodology.

^a Bootstrapped p -values (in parentheses) are based on 10,000 bootstrap replications, based on Cavaliere *et al.*'s (2012) methodology.

4 How Large Is the Size of the Unit Root in Real House Prices?

Having detected strong evidence of a unit root in real house prices, the next logical step is to investigate its *size*, that is, its importance in driving the series' fluctuations, compared to the role played by the stationary component. This will allow us to understand how strong mean-reversion in real house prices actually is, and what fraction of the period-on-period change in real house prices should be regarded as permanent. Following Cochrane (1988), I explore this issue based on the variance ratio, which for variable x_t , sample length T , and horizon k , is defined as

$$V_k = k^{-1} \frac{\text{Var}(x_t - x_{t-k})}{\text{Var}(x_t - x_{t-1})} \frac{T}{T - k + 1} \quad (1)$$

I estimate V_k as in Cochrane (1988), as

$$\hat{V}_k = \frac{T}{T - k + 1} \left[1 + 2 \sum_{j=1}^{k-1} \frac{k-j}{k} \hat{\rho}_j \right] \quad (2)$$

where the $\hat{\rho}_j$'s are the sample autocorrelations of the first difference of x_t . I construct confidence intervals for \hat{V}_k via the non-parametric spectral bootstrap procedure I used in Benati (2007), which is described in Section 2 of the online appendix.¹³ As I showed in Benati (2007) via Monte Carlo, this procedure has good coverage properties, in the sense of being able to effectively capture the authentic extent of uncertainty in the underlying data generation process.

Figure 2 reports the simple estimate of the variance ratio (that is, \hat{V}_k) at horizons up to 25 years ahead, together with the 16th, 84th, 5th and 95th percentiles of the bootstrapped distribution. Evidence suggests that, at the monthly frequency, the fraction of the period-on-period change in log real house prices which is due to the unit root component is equal to *about 13 per cent*, and it is quite precisely estimated, with a 90 per cent-coverage confidence interval stretching from about 7 to about 26 per cent. This points towards a dominant component of mean-reversion in real house prices, so that their month-on-month fluctuations should be regarded as overwhelmingly transitory. An important point to stress is that \hat{V}_k stabilizes, and it becomes essentially flat, after about 10 years, thus clearly pointing towards the reliability of the estimates.

I now turn to a comparison between the results produced by cointegrated VARs identified via long-run restrictions, and by VARs in levels identified as in Uhlig (2003, 2004), based on two alternative sample periods: the full sample up to 2017, and the shorter one excluding the Zero Lower Bound (ZLB) period. To anticipate, whereas—once focusing on the most powerful shock for real rents—results produced by VARs

¹³The online appendix is available at my webpage: <https://sites.google.com/site/lucabenatiswebpage/>

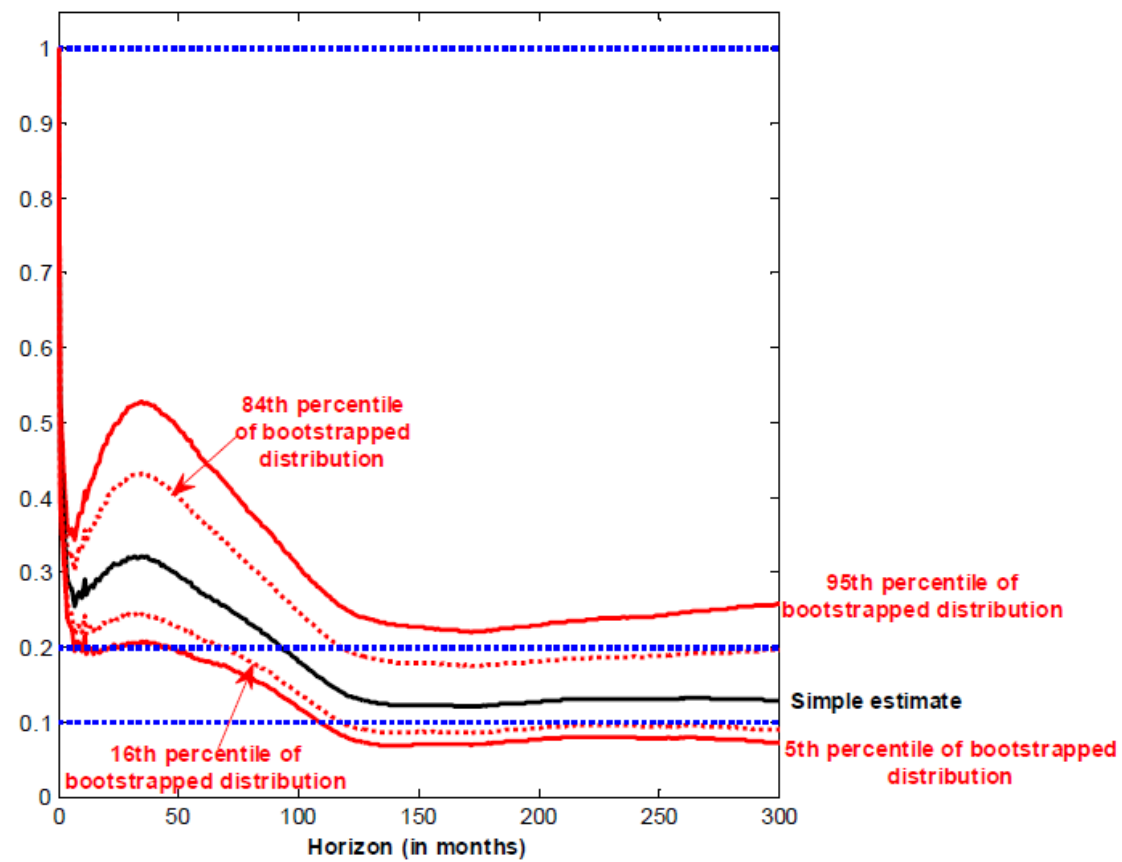


Figure 2 Estimates of the size of the permanent component of U.S. log real house prices based on Cochrane's variance ratio estimator

in levels are uniformly robust across the board, those produced by cointegrated are consistently weaker, and fragile along a number of dimensions.

5 Evidence from Structural VARs

Figures 3 and A.8 (in the appendix) report, for the full sample April 1971-August 2017, evidence based on a structural VAR (SVAR) in levels identified as in Uhlig (2003, 2004), and a cointegrated SVAR identified *via* long-run restrictions, respectively.¹⁴ In the former case, the permanent shock driving rents' and house prices' common stochastic trend has been identified as the shock explaining the maximum fraction of the fraction of forecast error variance (FEV) of real rents at the 25 years ahead horizon,¹⁵ whereas in the latter case it has been identified as the only shock having a permanent impact on log real rents. In either figure, the three panels show

(i) house prices, together with their estimated stochastic trend and bootstrapped confidence bands;¹⁶

(ii) the fraction of bootstrap replications for which, in each individual month, house prices' transitory component is estimated to have been positive; and

(iii) the estimated transitory component of house prices with bootstrapped confidence bands.¹⁷

Figures 4-6 and A.3-A.4 in the appendix are all based on the VAR in levels and the longer sample period: Figure 4 shows house prices' transitory component together with the unemployment rate; Figure 5 shows, for alternative horizons used for the identification of the permanent shock, the transitory component obtained when Uhlig's (2003, 2004) procedure is used to extract the most powerful shock for real *rents* and, respectively, real *house prices*; Figure 6 shows the transitory component obtained based on alternative lag orders (with the horizon used for identification set to 25 years ahead); and Figure A.3-A.4 show impulse-response functions (IRFs) to the permanent shock to real rents, and the fractions of FEV of individual series explained by the shock, respectively, based on the baseline setup (that is, the lag order set to 12, and the horizon used for identification set to 25 years ahead).

Figures A.9 to A.16 in the appendix are all based on VARs in levels: Figure A.9

¹⁴All VARs in levels have been estimated based on the eight series shown in Figure 1. As for the cointegrated VARs, for the reasons discussed in Section 3.2, results for the sample excluding the ZLB period have been based on the baseline 8-variables system, whereas those for the longer sample have been based on the 7-variables system excluding the supply of homes. In both cases, the lag order has been set to $p=12$. In Section 5.2 I explore robustness to alternative lag orders.

¹⁵As I discuss below, any 'target horizon' greater than or equal to 10 years ahead produces near numerically identical results.

¹⁶The cointegrated VECM has been bootstrapped as in Cavaliere, Rahbek, and Taylor (2012) conditional on the identified number of cointegration vectors, whereas the VAR in levels has been bootstrapped as in, e.g., Barsky and Sims (2011).

¹⁷In order to make the figures easier to read, all estimated objects have been smoothed *via* a 3-month rolling window.

shows the same results as in Figure 3, but based on the shorter sample excluding the ZLB period; Figures A.10-A.12 show the same results shown in Figures 4, A.3, and A.4, but based on the *owner's equivalent rent* component of the CPI,¹⁸ rather than based on the shelter component I use throughout the main body of the paper; Figures A.13-A.15 show the same results shown in Figures 4, A.3, and A.4, but based on identifying the most powerful shock for real *house prices* themselves, rather than for real rents; finally, Figure A.16 shows house prices' transitory components obtained by eliminating from the VAR one series (other than rents) at a time.

I start by discussing the main substantive results, and I then turn to issues of robustness.

5.1 Main substantive results

The main substantive findings can be summarized as follows.

First, the methodology proposed herein identifies two large, transitory deviations of real house prices from their estimated stochastic trend in the second halves of the 1970s and 1980s, respectively; a third, slightly larger deviation corresponding to the period immediately preceding the outbreak of the financial crisis and the Great Recession; and—inevitably tentatively—a fourth one at the very end of the sample.

Second, in all four cases, statistical significance is very high. In particular, focusing on the disequilibrium which pre-dated the Great Recession, the 84%, 90%, and 95% thresholds for the fractions of bootstrap replications for which the transitory component of house prices is estimated to have been positive were crossed in May, September and December 2004, respectively, and subsequently the fractions remained above the 90% threshold until November 2007. By the same token, during the most recent, and still ongoing episode, the fraction of bootstrap replications for which house prices are estimated to have been above their stochastic trend has consistently been beyond 90% since the end of 2016.

Third, the transitory component of house prices exhibits a strong negative contemporaneous correlation with the unemployment rate, and leads it by about four years (see the left and, respectively, right hand-side panels of Figure 4). The leading-indicator properties of transitory house prices for the unemployment rate emerge in an especially stark way from the episode associated with the Great Recession, with the fluctuations in the unemployment rate during the crisis closely tracking (up to a scale factor) the transitory fluctuation in house prices four years before.¹⁹ The interpretation of these stylized facts is however not straightforward. Consider, for example, the Volcker recession and the more recent Great Recession. Both episodes had been

¹⁸As discussed in online Appendix 1, the owner's equivalent rent component of the CPI is only available since January 1983.

¹⁹In principle, this is conceptually in line with Leamer's (2007) position that 'housing is the business cycle'. For an extensive discussion of the conceptual limitations of Leamer's (2007) position, see Smets' (2007) discussion of Leamer (2007).

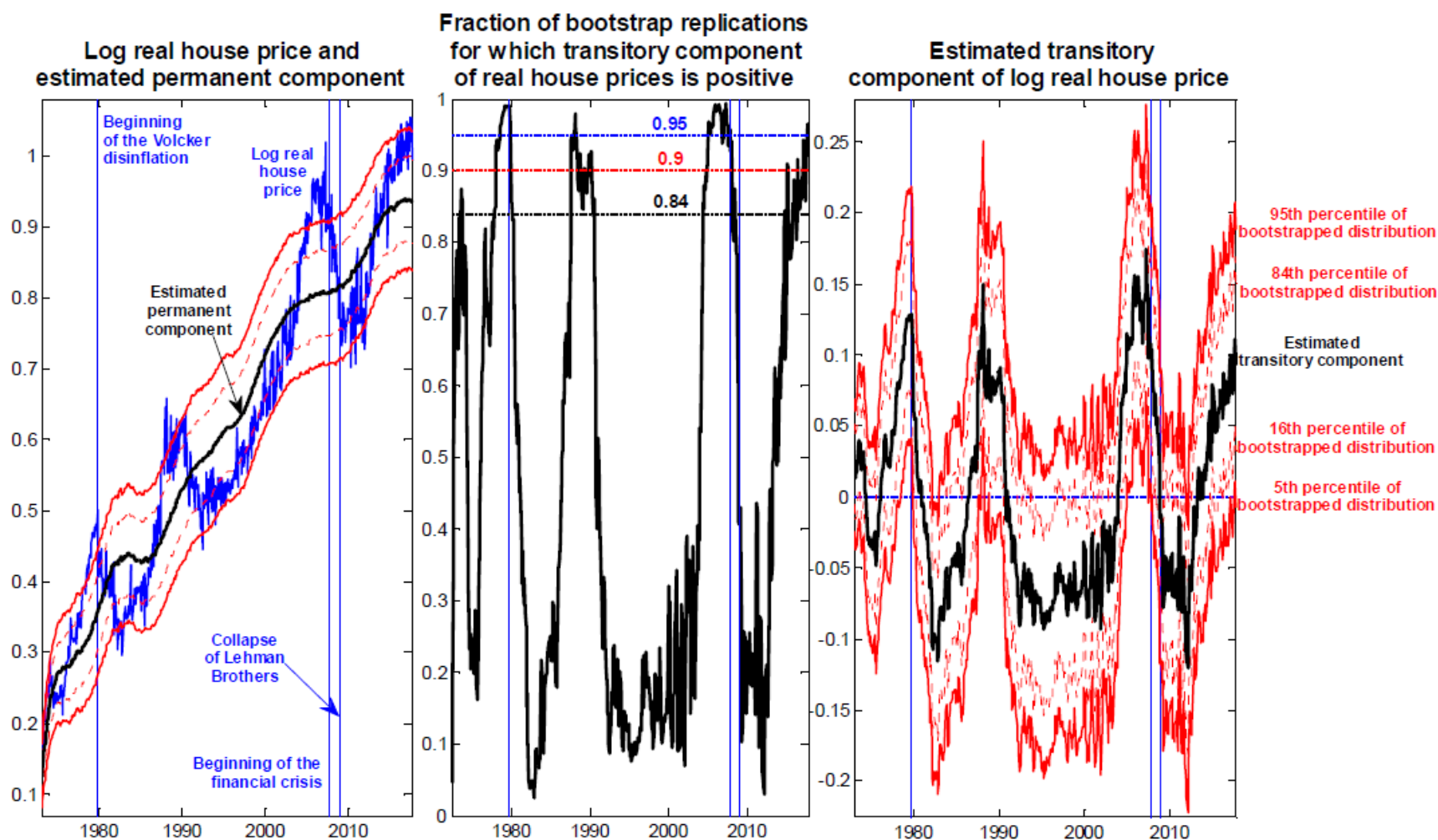


Figure 3 Results based on a VAR in levels identified as in Uhlig (2003, 2004): Estimated permanent and transitory components of log real house prices (estimates smoothed with a 3-month rolling window)

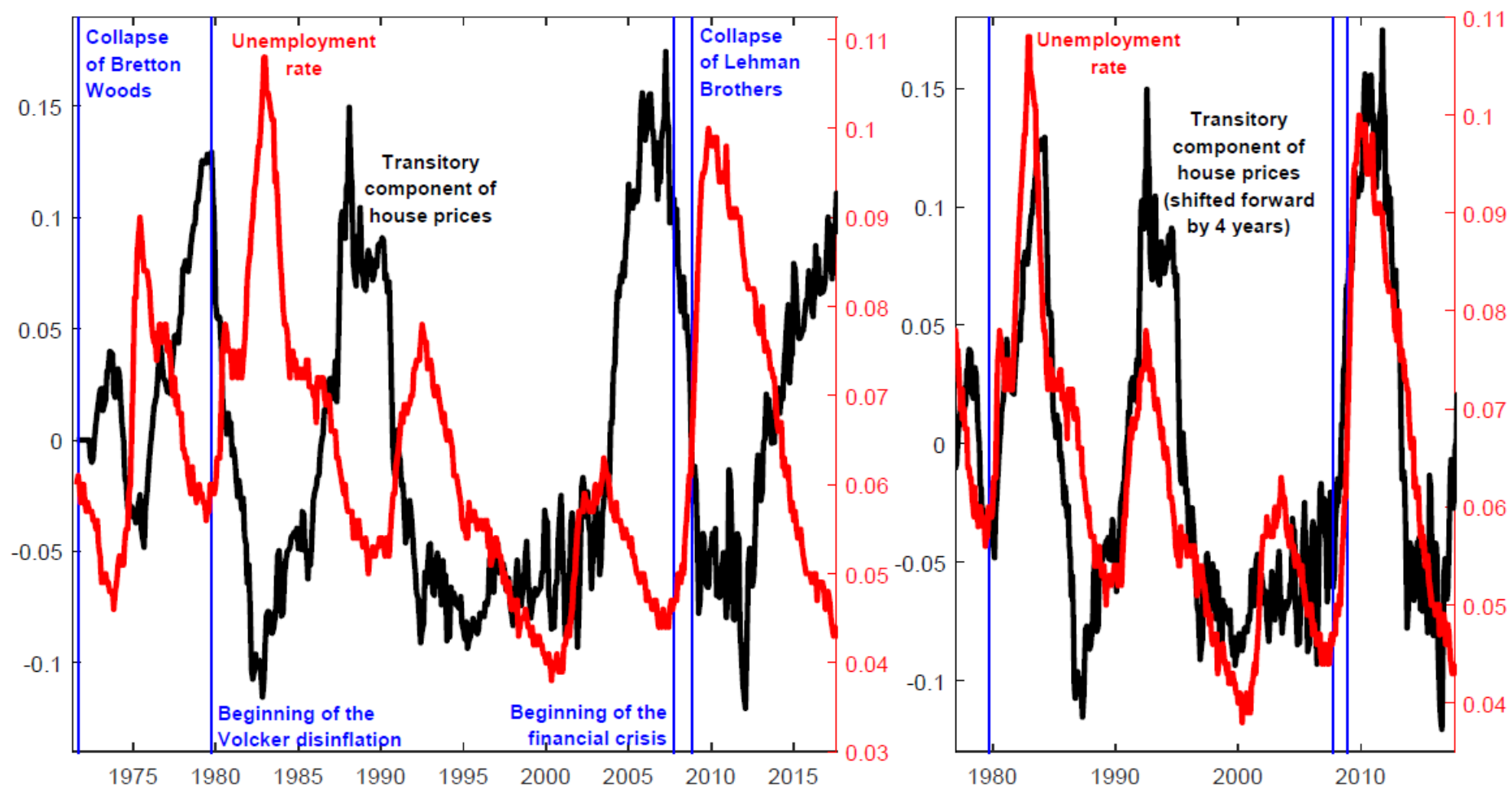


Figure 4 Results based on a VAR in levels identified as in Uhlig (2003, 2004): Estimated transitory component of log real house prices and unemployment rate

characterized by a crash in house prices—with the transitory component moving from strongly positive to strongly negative—and by a dramatic increase in the unemployment rate. In the former episode, however, most macroeconomists would agree that the direction of causality went from increases in the Federal Funds rate engineered by the FED in order to crush inflation, to *both* an increase in unemployment, *and* a collapse in house prices. In the latter case things are much less clear-cut. Part of the crash in house prices may have originated in a series of hikes in the Federal Funds rate which started in July 2004, but it is possible that a large, or even dominant portion of the crash simply reflected the bursting of the bubble. For the sake of the argument, let’s assume that the collapse in house prices *uniquely* originated from the bursting of the bubble, and that the subsequent increase in the unemployment rate was the consequence of the crash. Under this scenario, two superficially very similar patterns of correlation between house prices and the unemployment rate associated with two major recessions would in fact imply very different directions of causality. Very similar arguments can be made for several other superficially similar episodes, so that, in general, an interpretation of the evidence in Figure 4 is not clear-cut.

Fourth, the estimated over-valuation of real house prices during the period immediately preceding the outbreak of the financial crisis was not substantially greater than the corresponding over-valuations during the two previous identified episodes. Conceptually in line with, e.g., the analysis of Mian and Sufi (2014), and of Jordà, Schularick, and Taylor (2015), these results are therefore compatible with the notion that what made the recent housing bubble truly devastating was the fact that it had been developing concomitantly with the building up of massive amounts of household debt. On the other hand, the extent of over-valuation, *per se*, does not bear any clear-cut implication for the depth of a subsequent recession: In fact, the housing market disequilibrium of the second half of the 1980s was followed by the comparatively mild recession of the early 1990s, whereas the deep recession of the early 1980s is near-universally ascribed to Paul Volcker’s contractionary monetary policy, rather than to a housing market crash.

Fifth, at the 10-year horizon the permanent shock driving rents’ and house prices’ common stochastic trend explains nearly 90 per cent of the FEV of real rents, whereas it explains less than 30 per cent of the FEV of real house prices. This confirms the visual impression (see the first panel of Figure 1) that real rents are comparatively much closer to the common stochastic trend than real house prices, and it is in line with the evidence (see Figure 2) that house prices contain a dominant transitory component. Crucially, a comparison between Figures A.3 and A.14 in the appendix shows that results are qualitatively the same, and numerically very close, when Uhlig (2003, 2004)-style identification is used in order to extract the most powerful shock for *house prices* themselves, rather than for rents. This clearly shows that the relationship between rents and house prices is qualitatively the same as that between consumption and GNP, and dividends and stock prices, explored by Cochrane (1994). Finally, the identified permanent shock explains little-to-*nil* of the FEV of inflation, interest rates,

and housing starts; about 25-30 per cent of the FEV of the supply of homes at all horizons beyond two years ahead; and sizeable amounts—at the 10-year horizon, nearly half—of the FEV of employees in construction at all horizons beyond about three years ahead.

5.2 Robustness issues

Turning to robustness issues, the following main findings emerge from the relevant figures:

first, results based on cointegrated VARs are manifestly inferior to those based on VARs in levels along two important dimensions. (1) They are not robust to changes in the sample period (in particular, to including or excluding the ZLB period): This emerges very clearly from a comparison between Figure A.7 and Figure A.8, with the two sets of results being quite significantly different. The analogous comparison for the evidence based on VARs in levels, on the other hand (see Figures 3 and A.9), does not point towards any appreciable difference between results including and excluding the ZLB period. (2) The results produced by cointegrated VARs identified *via* long-run restrictions are systematically characterized by a greater extent of econometric uncertainty: This clearly emerges from a comparison between Figures 3 and A.8, and A.9 and A.7, respectively. *In the rest of the paper I will therefore exclusively focus on results produced by VARs in levels.*

Second, when Uhlig (2003, 2004)-style identification is used in order to extract the single most powerful shock at long horizons for real rents, *all* horizons greater than or equal to 10 years ahead produce near numerically identical results for the transitory component of real house prices (see Figure 5). Intuitively, this reflects the fact that real rents are, to a close approximation (and up to a scale factor), the stochastic trend of real house prices, so that all horizons which are ‘not too short’ allow to effectively capture the unit root in house prices. By contrast, when this approach is used in order to extract the most powerful shock for house prices themselves, alternative long horizons used for identification sometimes produce materially different results.

Third, VARs in levels produce very similar results based on either the shelter or the owner’s equivalent rent components of the CPI. Intuitively, this reflects the fact that (i) over the common sample period, the two CPI components have been nearly indistinguishable (see Figure A.1 in the appendix), and (ii) as mentioned, results produced by VARs in levels are robust to using alternative sample periods.

Fourth, alternative lag orders produce, most of the time, very similar transitory components of real house prices. In particular, in the left hand-side panel in Figure 6 only the transitory component based on $p=6$ sometimes materially diverges from those produced by the other lag orders considered. Since results based on either $p=12$ or $p=24$ are uniformly close (and they are close to those based on $p=3$), one possible ‘rule of thumb’ suggested by the left hand-side panel is to use a sufficiently large lag order. The fractions of FEV of real house prices explained by the identified

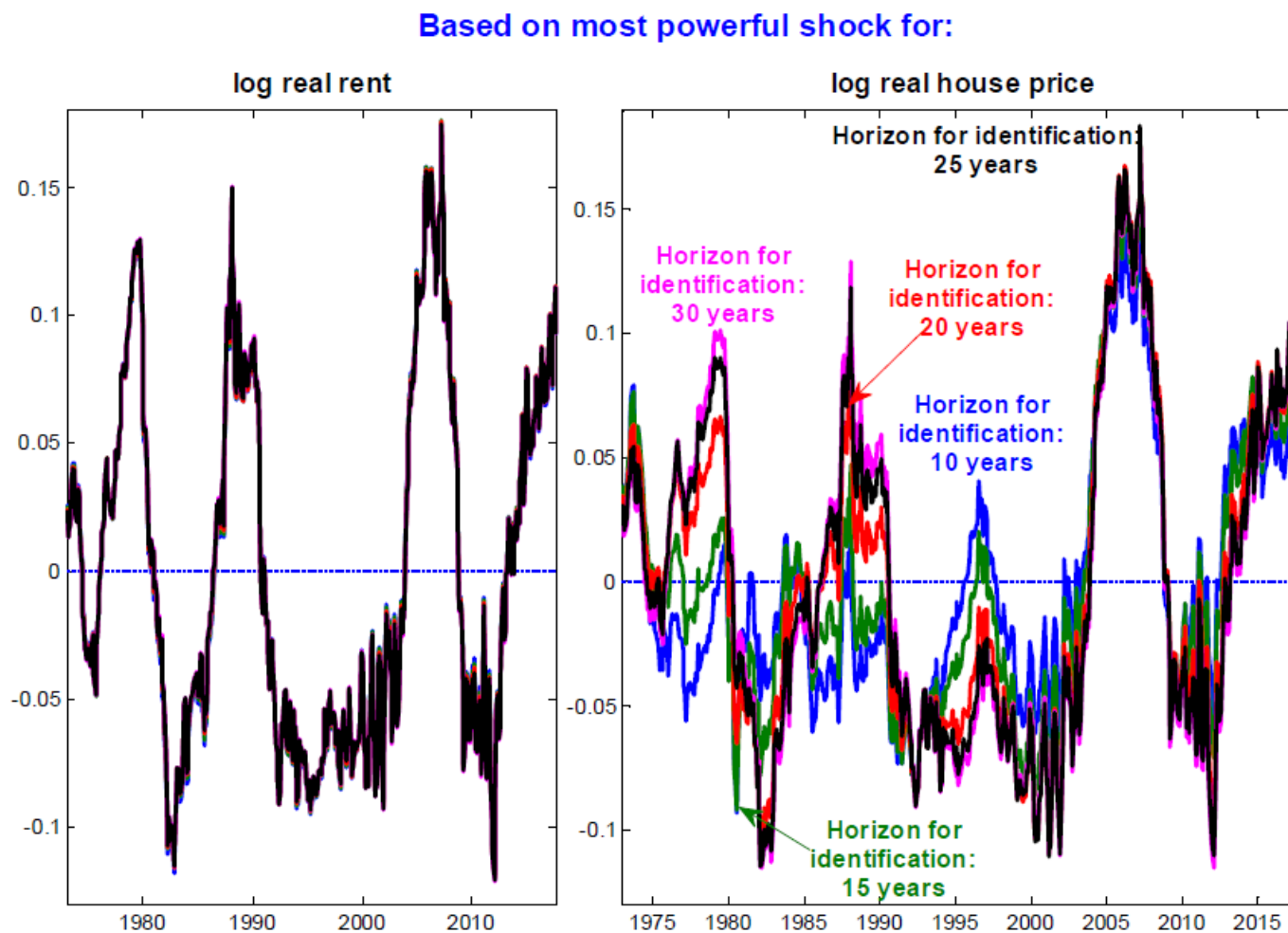


Figure 5 Results based on a VAR in levels identified as in Uhlig (2003, 2004): Estimated transitory components of log real house prices, for alternative horizons used for the identification of the permanent shock (estimates smoothed with a 3-month rolling window)

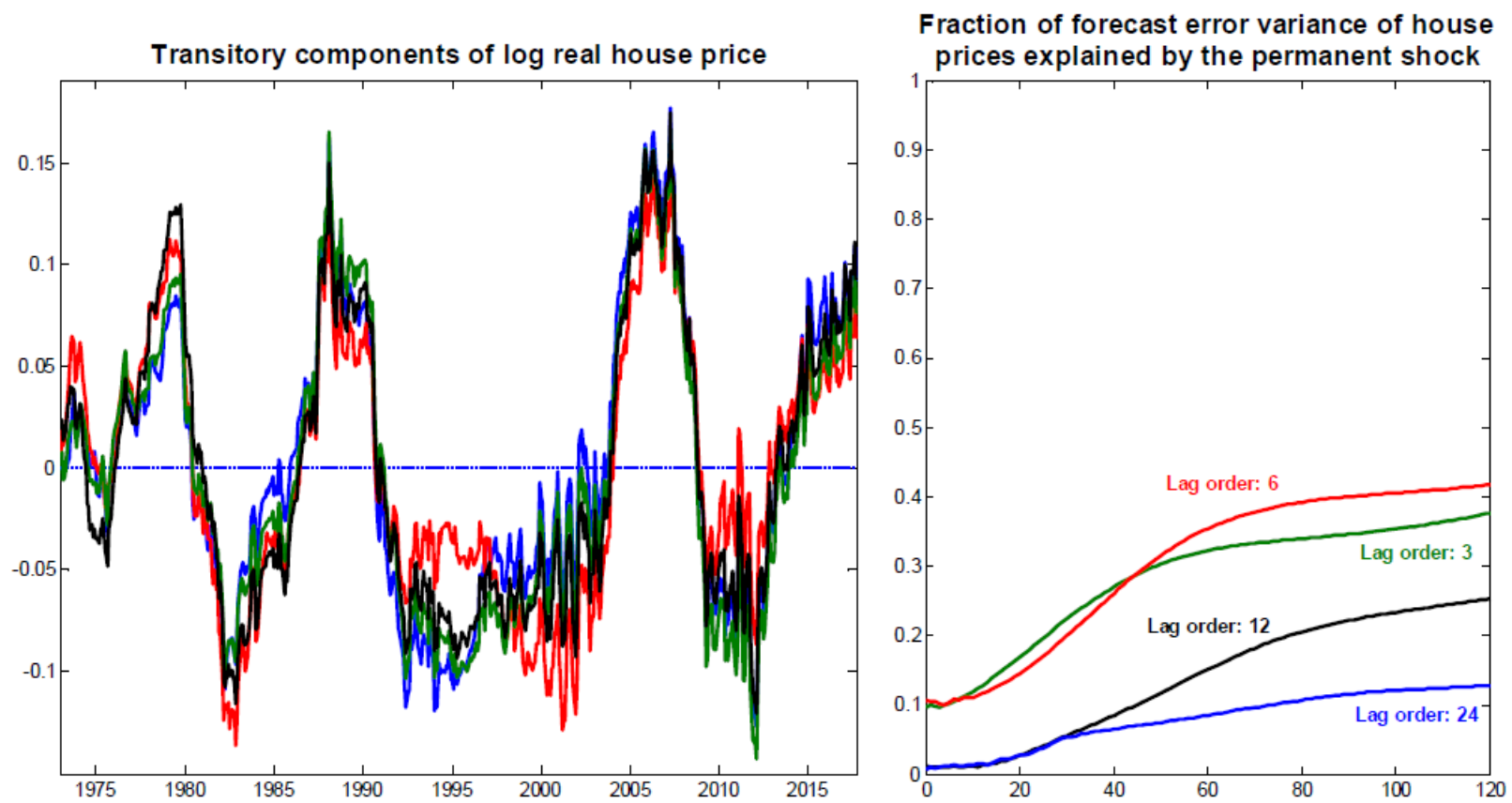


Figure 6 Results based on a VAR in levels identified as in Uhlig (2003, 2004), for alternative lag orders

permanent shock shown in the right hand-side panel provide further validation to this: As it was shown in Figure 2, the 90 per cent-coverage confidence interval for the size of the unit root in house prices stretches from slightly below 10 per cent to nearly 30 per cent. This means that whereas either $p=12$ or $p=24$ produce plausible values for the long-horizons fractions of FEV of real house prices explained by the permanent shock, both $p=3$ and $p=6$ produce values which are somehow too large. This provides an additional rationale for imposing, in estimation, a lag order at least as large as $p=12$.

5.3 Which features are key in order to robustly identify the transitory component of house prices?

For the purpose of using this methodology in order to robustly detect disequilibria in house prices, a crucial issue is identifying which features play a key role, and which ones are instead second-order. Beyond—as previously discussed—

- (i) using VARs in levels (as opposed to cointegrated VARs);
- (ii) applying Uhlig (2003, 2004)-style identification to real rents (rather than to house prices themselves); and
- (ii) using a sufficiently large lag order,

other features do not seem play a crucial role. In particular, series other than real rents, *considered individually*, appear to play a distinctly secondary role. As Figure A.16 (in the appendix) shows, indeed, eliminating these series from the VAR one at a time does not produce materially different estimates of the transitory component of house prices. At the same time, it is important to stress that eliminating *all* of them—thus ending up with a bivariate VAR for real rents and real house prices—produces dramatically different estimates of house prices’ permanent and transitory components (see Figure A.17). In particular, the transitory component is estimated to have been uniformly quite small, so that fluctuations in real house prices have been driven, to a significant extent, by permanent shocks. This is in stark contrast to the results in Section 4, where Cochrane’s variance ratio estimator suggested that the size of the unit root is between slightly below 10 and almost 30 per cent, with a point estimate of 13 per cent. Further, the transitory component of real house prices is estimated to have been significantly smaller, during the period pre-dating the Great Recession, than it had been during the second half of the 1970s, which appears as implausible. So, although, considered individually, series other than rents exhibit a limited informational content for the permanent component of house prices, once they are considered jointly their informational content is definitely non-negligible.

I now turn to the main substantive issue of whether this methodology would have allowed to detect the disequilibrium in U.S. house prices which pre-dated the Great Recession as it was building up.

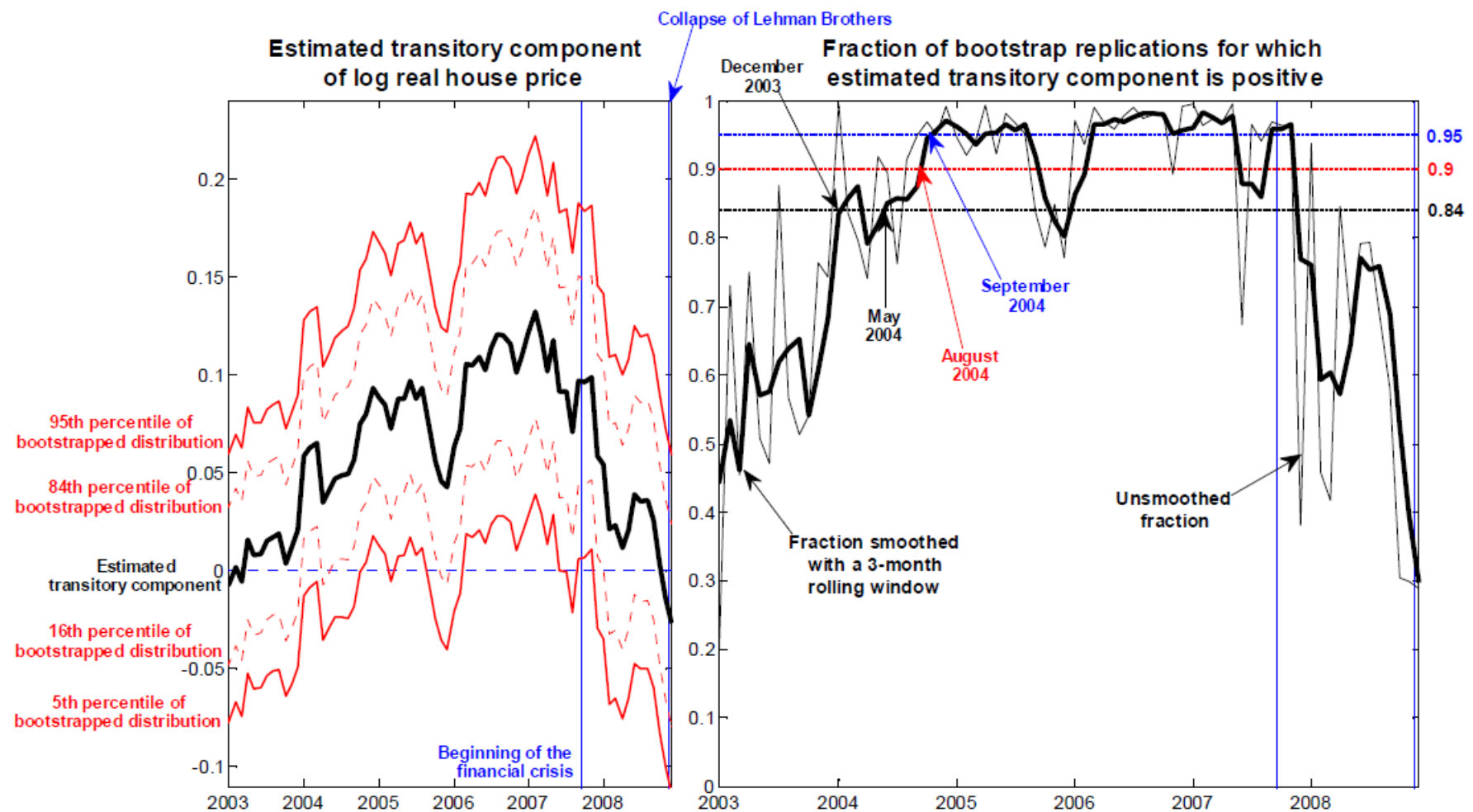


Figure 7 Results based on a VAR in levels identified as in Uhlig (2003, 2004): Partially real-time estimates of the transitory component of log real house prices, and fractions of bootstrap replications for which the transitory component is estimated to be positive

6 Could the Disequilibrium in U.S. House Prices Have Been Detected in Real Time?

Figure 7 shows the estimated transitory component of real house prices, and the fraction of bootstrap replications for which the component is estimated to have been positive, based on the following exercise. For each month τ , starting from January 2003, and up until October 2008 (i.e., one month after the collapse of Lehman Brothers), I estimate the same 8-variables VAR in levels I have discussed in the previous section based on the following data:

(i) for all series except real rents and the supply of houses, the *real-time data* which were available in month τ ;

(ii) for real rents and the supply of houses, the series available as of the end of the sample (i.e., August 2017).

The reason for this is the following. As discussed in the data appendix (Appendix 1), real-time data for rents and the supply of houses are available only starting from April and May 2011, respectively. In the next section I use these data in order to characterize the data-revision process for either series. Then, based on this, in Section 8 I perform an exercise near-identical to the one performed herein, with the only difference that, for real rents and the supply of houses, for each month τ I consider several ‘worst case scenarios’ in terms of what the revision noise might have been in that specific month. Specifically, by ‘worst case scenario’ I mean the *worst possible circumstances* for the purpose of identifying the house price ‘bubble’ in real time. Another way of saying this is that in Section 8 I will be ‘stacking the cards against myself’, considering a set of extremely challenging circumstances for the methodology analyzed herein. To anticipate, evidence clearly shows that, even in a worst case scenario in which the magnitude of revision noise had been either 2 or 3 times what it has historically been since 2011, my methodology would have detected the bubble with very high confidence four years before the collapse of Lehman Brothers. Intuitively, this has to do with the fact that, as I document in the next Section, for real rents—which, as previously discussed, is the key series for the purpose of identifying the unit root component of real house prices—the extent of revision noise is negligible, so that the first release is very close to the final estimate. (For the supply of houses, on the other hand, the extent of such noise is non-negligible, but this series plays a much less important role for the purpose of identifying the permanent component of house prices—see Figure A.16.) This implies that, for practical purposes, taking, or not taking into account of the revision noise for these two series makes little difference. Because of this, I have chosen to start by discussing the results from the present exercise, which—since it does not involve adding revision noise to the final estimates of rents and the supply of houses—presents the distinct advantage of being more transparent.

Two main results emerge from Figure 7:

first, the methodology analyzed herein would have detected, with high confidence,

the disequilibrium in U.S. house prices which pre-dated the Great Recession about three years before the outbreak of the crisis, in August 2007,²⁰ and four years before the collapse of Lehman Brothers. This is clearly apparent from the right hand-side panel, with the fraction of bootstrap replications for which the transitory component of house prices is estimated to have been positive crossing the 84, 90, and 95 per cent thresholds in May,²¹ August, and September 2004 respectively. Between September 2004 and the Summer of 2007, the fraction had then been consistently oscillating around or above the 95 per cent threshold, with the single exception of a temporary drop to a trough of 80 per cent in October 2005. Overall, with the single exception of this temporary decrease, the fraction had consistently remained high, or very high, during the entire period between the Summer of 2004 and the outbreak of the financial crisis.

Second, the extent of house prices' over-valuation had typically been non-negligible: By early 2006, the point estimate had crossed the 10 per cent threshold, with the 90 per cent-coverage confidence interval stretching to almost 20 per cent, and during subsequent months, up until the outbreak of the crisis, it oscillated around or above that level.

These results show that, in fact, the disequilibrium in U.S. house prices which pre-dated the Great Recession could have been detected with very high confidence four years before the collapse of Lehman Brothers. As discussed in the Introduction, the key point here is obviously *not* to '(re)litigate the past', and in particular the role played by the Federal Reserve during the period leading up to the financial crisis. Rather, my objective is to show, by example, that house prices' disequilibria *can* in fact be detected as they are building up based on standard time-series methods exploiting the long-run equilibrium properties of the housing market.

The present work is therefore an illustration of, and provides validation to, the criticism the *Economist* magazine laid out, starting from the second half of the 1990s, to Alan Greenspan's position that 'bubbles cannot be detected'. The *crux* of the *Economist's* argument (see the opening quotation) was that there is nothing special about asset prices, and, in the same way as central banks routinely estimate unobserved economic objects such as potential GDP and the natural rate of unemployment—and take monetary policy decisions based on them—the same could be done for stock and house prices.

I now turn to analyzing the revision process for real rents and the supply of homes.

²⁰The outbreak of the financial crisis is typically taken to be early August 2007, when the European Central Bank started massively intervening in Euro area money markets.

²¹In fact, the 84 per cent threshold had first been crossed in December 2003, but until May 2004 the fraction had been oscillating around that level, therefore providing a less decisive signal of house prices overvaluation.

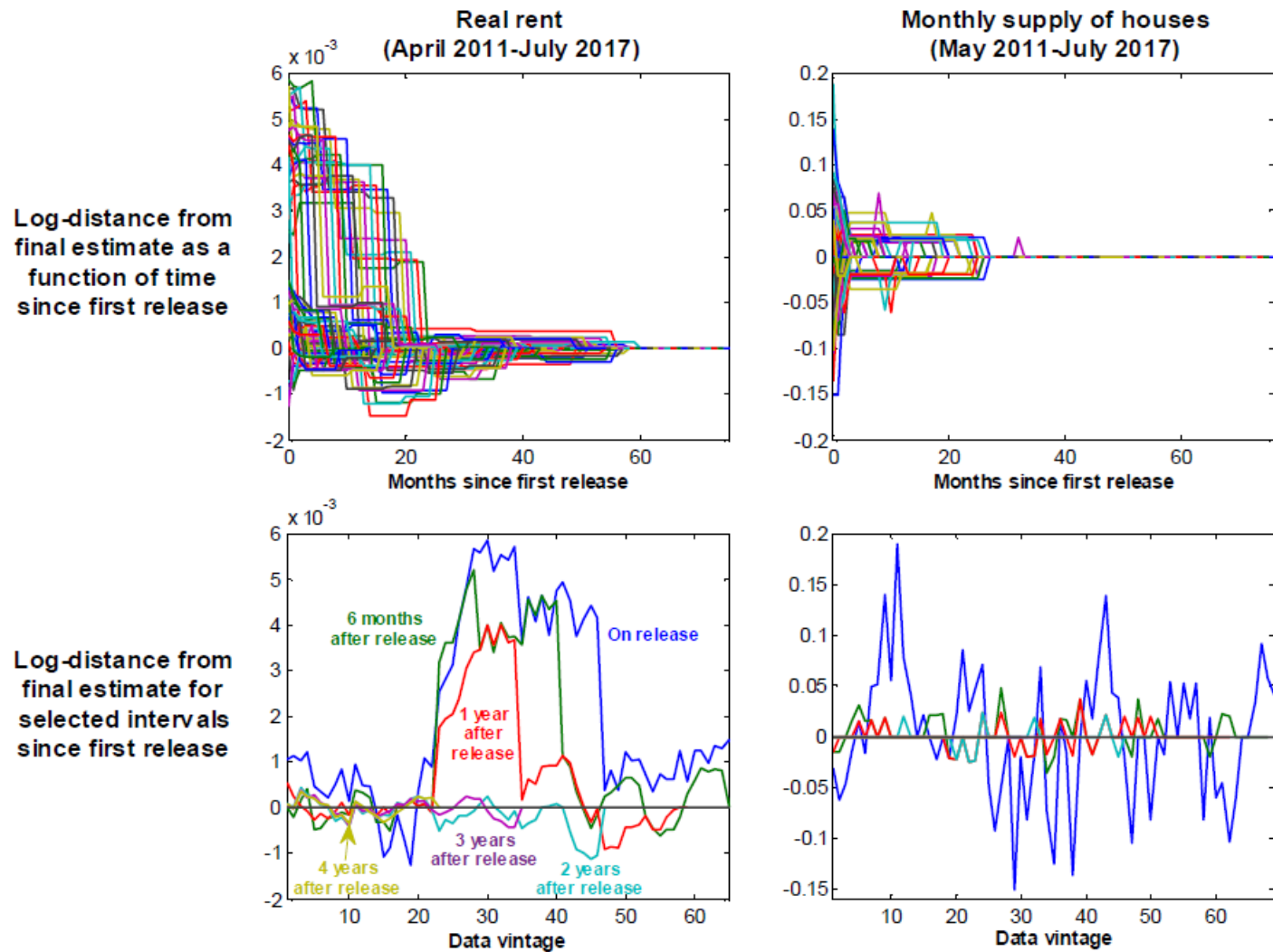


Figure 8 Features of the data revision process for the real rent and the monthly supply of houses

7 The Revision Process for Real Rents and the Supply of Homes

Figure 8 illustrates the revision process for real rents and the supply of homes since 2011, when real-time data for the two series first start being available.²² Specifically, the top row shows, for either series, the log-distance from the final estimate—defined as the difference between the logarithms of the *final* estimate and of the *current* estimate—as a function of the time (in months) which has expired since the first data release. The following main facts emerge from the two top panels:

First, the extent of revision noise for real rents—the crucial series for the purpose of robustly identifying the permanent component of real house prices—is *two orders of magnitude smaller* than the corresponding noise for the supply of homes, and it is essentially negligible. This means that, for rents, the initial data release is very close to the final estimate, so that, for practical purposes, the latter can be regarded as essentially equivalent to the former. For the supply of homes, on the other hand, the extent of revision noise is not negligible, but, as the exercise of Section 8 will show, this is not a problem, since this series plays a less important role in identifying the unit root in house prices.

Second, for the supply of homes the revision process ends about two years after the initial release. For rents, on the other hand, the process lasts longer, but given the negligible extent of revision noise this is, once again, essentially irrelevant for practical purposes.

The bottom row of Figure 8 characterizes a different aspect of the revision process for the two series, with the two panels showing, for all available months since when real-time data start being available, the log-distance from the final estimate both on release, and for selected time intervals after release. The main finding emerging from the two panels is that whereas for the supply of homes the log-distance from the final estimate is essentially zero-mean, for rents this is not the case: In particular, at short ‘horizons’ (i.e., until one-two years after the first release) the distance has been mostly positive, thus implying that, since April 2011, the initial release for real rents has typically been ‘too high’.

In the real-time exercise in Section 8 I will assume that, for either series, the revision process since 2011 shown in Figure 8 provides a good characterization of the revision process for the period of interest (January 2003–October 2008). In particular, I will assume that the *magnitude* of the revision noise for the period January 2003–October 2008 is well captured by the magnitude for the period since 2011. (As I discuss in Section 8, for robustness reasons I will then also consider exercises in which I scale up the revision noise since 2011 by a factor of either 2 or 3.) Although I have no hard proof that this is indeed the case, I regard this assumption as reasonable, as it

²²To be precise, as discussed in the data Appendix 1, real-time data for the PCE deflator are available since August 2000. The problem is that real-time data for nominal rents are only available since April 2011.

appears as implausible that the revision process for either series may have materially changed since 2008. *Prima facie* evidence that the assumption is a plausible one is provided by the fact that for *all* of the other six series in the VAR, for which real-time data are available at least since the early 2000s, the revision process clearly appears to have remained unchanged since then. Specifically, neither the Federal Funds rate nor the 30-year mortgage rate are *ever* revised. As for the remaining four series, the evidence in Figure A.18 in the appendix provides no support whatsoever to the notion that their revision process may have materially changed over the period August 2000-September 2008.²³

I now turn to discussing in detail the results from the real-time exercise.

8 A Real-Time Exercise with a Worst-Case Scenario for the Revision Noise for Rents and the Supply of Homes

Figure 9 shows the same evidence as in Figure 7 (i.e., the transitory component of real house prices, and the fraction of bootstrap replications for which the component is estimated to have been positive), based on an exercise which is identical to that performed in Section 6, except for the following.

In Section 6 I used, for real rents and the supply of houses, the series available at the end of the sample, which is equivalent to assuming that neither series is ever revised, so that the initial release is identical to the final estimate (as mentioned, given the very small extent of revision noise for real rents, for this series the assumption is quite close to the truth). Here, on the other hand, I take into account of the existence of revision noise for either series as follows. Consider the generic real-time recursive sample ending in month τ , with $\tau \subseteq [\text{January 2003; October 2008}]$. For the purpose of detecting the house prices disequilibrium *during this specific period*—in which the ‘bubble’ was inflating, so that house prices were creeping up compared to rents—the worst case scenario is one in which both real-time rents, and the real-time supply of houses, were systematically *higher than the final estimates*, so that real-time revision noise (defined as the difference between the real-time estimate available in month τ and the final estimate) was, for either series, *consistently positive*. The reason for this is straightforward. As for real rents, the higher the real-time rent compared to the final estimate, the lower the real-time ratio between house prices and rents, and therefore the more the SVAR would interpret the increase in real house prices as permanent, as opposed to transitory. In the limit, if the real-time revision noise for real rents had been consistently positive during the entire period between January 2003 and October 2008, and sufficiently large, the SVAR would have interpreted the ‘bubble’ as entirely

²³I restrict the analysis to the period starting in August 2000 because real-time data for the PCE deflator are first available for the August 2000 vintage.

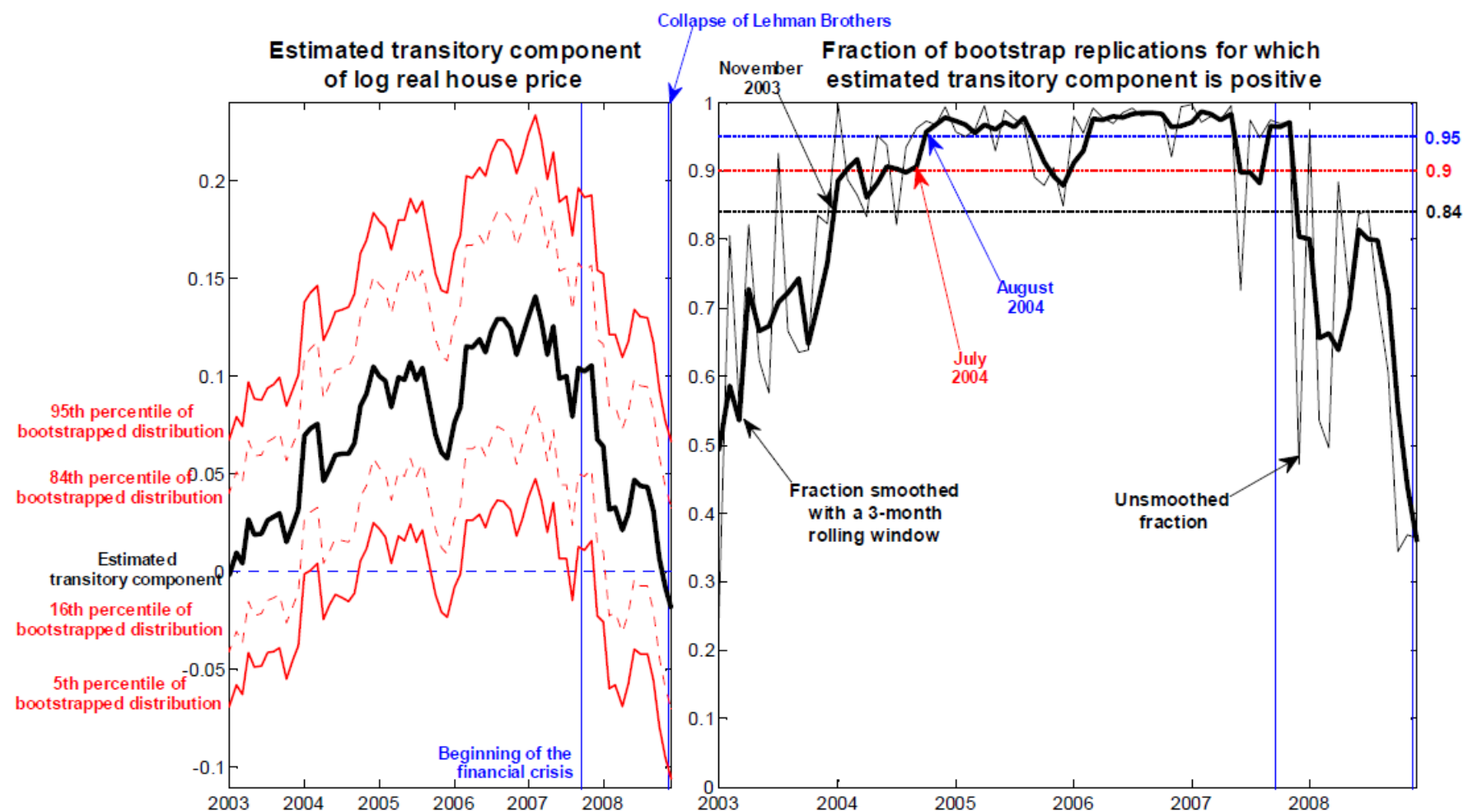


Figure 9 Results based on a VAR in levels identified as in Uhlig (2003, 2004): Real-time estimates of the transitory component of log real house prices, and fractions of bootstrap replications for which the transitory component is estimated to be positive, in the ‘worst-case scenario’ for the ‘revision noise’ for real rents and the supply of homes

due to permanent shocks. As for the supply of homes, the reason is that this series' fluctuations exhibit a remarkable coherence with the transitory component of real house prices, but tend to lag it by about two to three years. As a result, if, during the building up of the house prices' 'bubble', the supply of homes had systematically been higher than the final estimate, the SVAR would have interpreted this as evidence that movement in real house prices were driven by permanent, as opposed to transitory shocks. So the bottom line is that, for either series, the 'worst case scenario' for the purpose of detecting the disequilibrium in real time would have been one in which the series' real-time estimate in each month τ had been systematically higher than the final estimate.

I therefore proceed as follows. I compute, for either series, and for each month $m = 0, 1, 2, \dots$ since the first release, the *maximum* amount of revision noise which has historically occurred over the periods April 2011-July 2017, and May 2011-July 2017, respectively. To be clear, such maximum amount of revision noise is simply the maximum, for each month $m = 0, 1, 2, \dots$ since the first release, of the series plotted in the top row of Figure 8 (i.e., the log-distances from the final estimate as a function of the time elapsed since the first release). Let $n_m^{rr} \geq 0$ and $n_m^{sh} \geq 0$ be the maximum amount of revision noise for real rents and the supply of homes, respectively, for month $m = 0, 1, 2, \dots$ since the first release. Given the *final estimate* of log real rents up to month τ , $rr^\tau = [rr_1, rr_2, rr_3, \dots, rr_{\tau-2}, rr_{\tau-1}, rr_\tau]'$, I construct a 'worst case scenario' *real-time* estimate as $rr_{WCS}^\tau = [rr_1 + n_{\tau-1}^{rr}, rr_2 + n_{\tau-2}^{rr}, rr_3 + n_{\tau-3}^{rr}, \dots, rr_{\tau-2} + n_2^{rr}, rr_{\tau-1} + n_1^{rr}, rr_\tau + n_0^{rr}]'$. For the supply of homes I construct a 'worst case scenario' real-time estimate sh_{WCS}^τ in the same way. Finally, I repeat the real-time exercise of Section 6 substituting the final estimates of rents and the supply of homes I had there with the 'worst case scenario' real-time estimates rr_{WCS}^τ and sh_{WCS}^τ I just mentioned.

The results are reported in Figure 9: The key finding is that evidence is virtually the same as in Section 6. Intuitively, this has to do with the fact that, as previously mentioned, the extent of revision noise for the key series, real rents, is essentially negligible, so that even if we consider its maximum since 2011 for each month $m = 0, 1, 2, \dots$ since the first release, it ultimately does not make any material difference.

Finally, I also consider two alternative 'extreme worst case scenarios' in which I multiply both $n_m^{rr} \geq 0$ and $n_m^{sh} \geq 0$ by a factor of either 2 or 3. It is worth stressing that these two cases correspond to an extent of revision noise which is *way* outside the realm of what we have historically seen since 2011. The results are reported in Figures A.19 and A.20 in the online appendix. Once again, even such an historically anomalous amount of noise does not change results in a material way: The methodology proposed herein would still have been able to detect the disequilibrium in house prices in real time.

9 Conclusions

Since the Great Recession was triggered by the unravelling of a large disequilibrium in U.S. house prices, one would logically expect that—in the same way as the macro-economic profession has devoted a vast effort to expanding DSGE models in order to incorporate a financial and a banking sector—an analogous effort would have been devoted to developing methods allowing to reliably identify house prices’ disequilibria in real time. Quite surprisingly, this has not been the case. As a consequence, although policymakers often openly worry that housing markets may be heading into ‘bubble territory’, they have no way of assessing such a possibility within a rigorous statistical framework. As a consequence, they are unavoidably compelled to resort to intuitively sensible, but ultimately quite rough indicators such as the rent-price ratio, or the ratio between house prices and incomes.

In this paper I have explored whether time-series methods exploiting the long-run equilibrium properties of the housing market might have detected the disequilibrium in U.S. house prices which pre-dated the Great Recession as it was building up. Based on real-time data, I have shown that a VAR in levels identified as in Uhlig (2003, 2004) would have detected the disequilibrium with high confidence by the Summer of 2004, with the estimated extent of overvaluation peaking at about 15 per cent immediately before the crisis. Cointegrated VARs identified *via* long-run restrictions, on the other hand, tend to produce fragile and uniformly weaker results. Conceptually in line with Cochrane’s (1994) analysis for consumption and GNP, and dividends and stock prices, the single most important factor in order to reliably and robustly identify the transitory component of real house prices is applying Uhlig (2003, 2004)-style identification to real rents, which are cointegrated with house prices, and are comparatively much closer to the common stochastic trend. In particular, when Uhlig-style identification is used in order to extract the single most powerful shock at long horizons for real rents, all horizons greater than or equal to 10 years ahead produce near numerically identical results for the transitory component of real house prices. By contrast, when this approach is used in order to extract the most powerful shock for house prices themselves, alternative long horizons used for identification sometimes produce materially different results.

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